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COGNITIVE PATTERNS IN THE EMPIRICAL SCIENCES: EXAMPLES OF COGNITIVE STUDIES OF SCIENCE

Theo A.F. Kuipers

"Roughly, I maintain that there is an empirical, descriptive (but not merely descriptive) 'science of science'."
(J.D. Sneed)

Introduction

Traditional philosophy of science is usually rather abstract and not very realistic. This is certainly one of the main reasons that its results, if any, seldomly have clear practical use value. If examples are analysed at all, it frequently concerns everyday matters such as "How many black ravens one should have observed in order to be allowed to draw the conclusion that all ravens are black?"

Partly in reaction to the abstract character of philosophy of science, there arose, from the beginning of the seventies, when Kuhn's *The structure of scientific revolutions* became popular, all kinds of social science oriented studies of science. Whatever one might think of some specific examples and declarations of relativist policy, one has to concede in general that these studies produced pictures of science in practice which are more concrete and realistic than ever before.

On the analogy of social studies of science there is gradually arising a program of cognitive studies of science, which can be localized somewhere between abstract philosophy of science and social studies of science. As a first global characterization the following circumscription can suffice: quasi-empirical studies of cognitive aspects of scientific knowledge and its development. The qualification 'quasi-' (empirical) is of course used to leave room for normative problems and aims. By consequence, the research is frequently quided by one or more heuristic-normative points of view, e.g. the intention to extrapolate patterns of successful

cases to problematic cases. Abstract philosophy of science (verbal as well as formal) has few resemblance with an empirical science. At best it sticks to respectable idealizations, at its worst it degenerates into so-called interesting philosophical debates. In contrast to this, cognitive studies of science are intended as contributions to a genuine empirical science (including theories!) or, better, an empirical *meta-science*.

The core of the program is of course the idea that there is system in knowledge and its development, and hence that theory formation about it is in principle possible. Of course, it concerns part and aspect systems and hence different patterns, which may or may not connect in a harmonious way. The best way to give some insight in cognitive studies of science hence is to present in a, by necessity, sketchy way a number of representative examples. This will be done with a rather immodest emphasis on my own research and that of other people that are or have been affiliated to the Groningen Science Studies Unit.

In this paper I will subsequently deal with the following topics:

- 1) explanation of laws by theories,
- 2) intentional and functional explanation,
- 3) interdisciplinary research,
- 4) theory development guided by interesting theorems,
- 5) theory development guided by idealization and concretization,
- 6) the structuralist perspective and truth approximation,
- 7) the structure of theories,
- 8) from material to an industrial product,
- 9) similarity and the choice of an inductive statistical method.

About all these subjects there have been found, on the basis of case-studies, more or less concrete and realistic patterns. It always concerns informative patterns that seem useful in one way or other, or that have already been used successfully. It is instructive to distinguish at least the following five kinds of possible use value:

- a) providing the 'null hypothesis of the ideal course of events', which can play a guiding role in social studies of science,
- b) clarification or even solution of problems belonging to abstract philosophy of science,
- c) improvement of the presentation in advanced textbooks, leading to better understanding and remembrance,
- d) playing a heuristic role in ongoing and new scientific research,
- e) playing a heuristic role in local research policy and global science policy.

For each subject the main use value aspects of the presented patterns will be indicated. As already suggested this will frequently have to be done in terms of pretensions which still have to be proved.

1. *Explanation of laws by theories*

Although "explanation" is one of the most favourite topics in traditional philosophy of science, the results of analysis that one can find in philosophy of science textbooks are not very informative. As far as nomological explanation is concerned, i.e. explanation on the basis of laws and theories, the standard analysis consists of an *unanalyzed* (one step) argument for the explanation of an individual fact, with the addition that a somewhat different form applies in the case of explanation of general facts, i.e. laws, and that probabilistic, or better approximative, variants are also possible.

For at least two reasons this is meagre. In the first place it can easily be checked that the natural sciences deal most of the time, and the social sciences frequently, with the explanation of (quasi-)laws on the basis of theories, with the explanation of individual facts given into the bargain. In the second place, even global knowledge of some concrete examples of scientific explanation is sufficient to conjecture that such examples can be decomposed into a fairly small number of standard steps.

Recently I have formulated a five steps model for the explanation of laws by theories (1 a/b) which was inspired by earlier detailed analyses of an example from physics and one from sociology (1 c/d). My claim is that generally accepted explanations of laws always consist of one or more of the following steps: application, aggregation, identification, correlation and approximation. Each step requires its own specific auxiliary hypotheses. Globally the steps amount to the following, where we assume for a moment that all steps occur. In the *application step* the theory is tailored to the kind of objects the law is about, explicitly or implicitly. This requires specification auxiliary hypotheses. In the *aggregation step* the total effect is calculated of the individual law derived in the first step. This requires statistical auxiliary hypotheses. In the *identification step* the resulting law is transformed with the aid of some identificatory auxiliary hypotheses connecting terms of the theory with terms occurring in the law. In the *correlation step* a similar transformation is undertaken, but now on the basis of causal auxiliary hypotheses. Finally, in the *approximation step* the thusfar obtained law is turned into the intended law on the basis of some idealizing auxiliary hypotheses. In contrast to the foregoing steps, the last step is a non-deductive one.

The five steps are summarized in the following scheme.

application	<u>T</u>	<u>A1</u>
aggregation	<u>L1</u>	<u>A2</u>
identification	<u>L2</u>	<u>A3</u>
correlation	<u>L3</u>	<u>A4</u>
approximation	<u>L4</u>	<u>A5</u>
		L5

Thusfar, at least ten examples from different sciences turned out to fit in the model in detail. Of course I do not exclude that the model may turn out to be too narrow for other examples. This would be unproblematic as long as the number of steps to be added remains small.

To indicate the use value of the model, I run through the five use value aspects:

(a) Hypothesis: controversies about particular explanations persist, among others, as long as none of the parties follows (implicitly) the steps model.

(b) The discussion in philosophy of science about the nature of reduction is so confused because three meanings are interwoven: on closer inspection the occurrence of one of the steps aggregation, identification or approximation turns out to be sufficient in the literature to speak of reduction.

(c) Explanations in advanced textbooks guided by the model will improve understanding and remembrance.

(d) The model can play a heuristic role in actual research when one is looking for an explanation of a law by a theory, in the context of a controversy or not, by suggesting in some more detail what one is looking for.

(e) Hypothesis: in research policy it is possible to deal with persisting controversies by stimulating research in the line of (d).

Claim (b) is elaborated in (1 a/b), claim (c) is illustrated there by ten examples, while the earlier mentioned examples from physics and sociology are dealt with in some detail.

There are at least two current research projects in the sense of (d): Rick Looijen is dealing with explanations in biology (2) and Maarten Janssen is dealing with the so-called micro-foundation of macro-economics (3). Both projects can at the same time be viewed as tests of hypotheses (a) and (c).

2. *Intentional and functional explanation*

The 'covering-law model' of explanation has often been criticized because it is claimed not to do justice to, for example, intentional explanation in history and functional explanation in biology. Authors like Hempel and Nagel have tried very hard to bring these two types of explanation in nomological form. Their conclusion roughly is that it is possible in principle, but that biology, history and all other sciences can and have to learn still very much from physics and chemistry. This caused a bad conscience or even an inferiority complex of many scientists using intentional or functional explanations.

Against this background it is very refreshing to start from the idea that most biologists and historians are not failed scientists and that their explanations have to be taken seriously. For example that sticklebacks really move their breast fins in order to get oxygen rich water into the nest and that Henry VIII indeed wanted to divorce and remarry in order to get a male descendant. Taking seriously means of course: looking for system in their way of explanation, without the preconceived idea that it should lead to some form of the covering-law-model. When this succeeds it is of course a contribution to the emancipation of the respective sciences.

From this point of departure I have first made separate analyses of intentional and functional explanation (4a/b). Later it became clear that the discovered patterns are special cases of a general pattern of explanation consisting of the specification of a certain qualification (intentional, functional, etc.) of a fact, for which reason I have called this general pattern *explanation by specification* (4c). The main ingredients of such an explanation are in the first place a decomposition key of the meaning of specific statements in which the qualification occurs, e.g. that a person had such and such intention with a particular action, that a particular trait of a species has this and this function, and in the second place the existential generalization of such statements, such as: this action was intentional, that trait is functional, etc. On this basis it is possible to formulate a hypothetical-deductive train of thoughts that is in line with the practice of historians and biologists. The differences concern of course the meaning decomposition keys, in which for example in the case of intentional explanation no appeal to a general law is required where-

as this is necessary in the case of functional explanation, but in a completely different way than the one prescribed by the covering-law-model.

Restricting myself to intentional explanations and to the most naive meaning decomposition the train of thoughts amounts to the following, when maximally successful in one round:

1. Observation: person x performed action a .
 2. *Why-question*: why did x perform a ?
- Heuristic phase
3. Unspecific hypothesis: x performed a intentionally?
 4. Specific hypothesis: x performed a in order to approach goal g ?
From the naive key now follow the following subhypotheses:
did x wish g ? and did x think that a was useful for g ?
- Test phase
5. Suppose that all subhypotheses are verified and hence also the specific hypothesis, then:
Why-answer: x performed a in order to approach g !
 6. Side-step with existential generalization: x performed a intentionally!
 7. Direction of attention to subsequent questions as: why did x wish g ?

I conclude again with the use value aspects

(a) Hypothesis: between intentional explanation and functional explanation (in biology) there are in the social sciences intermediate cases which can or cannot be clearly distinguished, which are characteristic for the style of scientific communities, and which easily lead to communication disturbances. Second hypothesis: discussions about autonomy and reductionism in biology could be enlightened very much in terms of different, but not necessarily incompatible, types of explanation.

(b) The standard belief in philosophy of science that the conclusion of the intentional or functional explanatory argument is that the fact to be explained will or has to occur, is false. The supposed underlying argument concerns existential generalization (step 6). Causal explanation of specific events, of which it is also disputed that they conform to the covering-law-model, turns out to be also conceivable as a special case of explanation by specification (4c). Yao Hua Tan investigates among others the thus resulting connection with (AI-)analyses of explanation on the basis of insufficient information (5).

(c) Alfons Keupink has shown that the argument of F. Fischer for his famous

theses about the origin of the First World War can be enlightened very much in terms of explanation by intentional specification (6).

(d/e) Hypothesis: Lex Guichard argues that animal stress researchers would profit a lot from conceiving explanation by (quasi-) intentional specification as a guide program, in the sense of the next section, in the study of animal behaviour (7).

3. Interdisciplinary research

As is well-known according to Popper a new theory implies only real progress in science if it successfully predicts new facts. In the line of this, Lakatos calls a research program only progressive if it generates from time to time theory transitions which anticipate the facts. Although there are many impressive historical examples, with the transition from Newton to Einstein as one of the paradigm cases, there are two problems with this strong definition of progress. In the first place it is difficult to see from the point of view of truth approximation (see Section 6) why theory accommodation to new facts cannot lead to the truth. In the second place there are many areas of twentieth century natural sciences where anticipation of new facts almost never occurs, whereas the relevant programs are certainly successful if one looks at the amount of money spent to it or at the awarded Nobel prizes.

Henk Zandvoort (8) tested the descriptive claim of Popper and Lakatos by investigating the main theory transitions in the nuclear magnetic resonance (NMR-) program (originating from nuclear physics, and based on quantum mechanics) and had to conclude that in almost all cases it concerned theory accommodation on the basis of newly discovered facts. On closer inspection it became also clear that nobody seriously doubted the possibility that the NMR-theory could explain the new facts by some further articulation and hence that such doubt could not be the reason why the program was prolonged. In fact, the program was continued because there were good reasons to assume that it could lead to important contributions to other research programs, in particular in chemistry and biology. Almost all these expectations came true later on.

In this way the case of NMR became for Zandvoort the paradigm case for a specific model for successful interdisciplinary research (IR)

IR-model: interdisciplinary research consists of some research programs, belonging to one or more disciplines, cooperating according to the following rules of the games:

- one program is the *guide-program* which raises problems of theoretical or experimental nature to the others,
- the other programs are service- or *supply-programs*, which have successfully passed their test phase and hence can try to solve the problems provided by the guide-program.

Compared with the popular ideas about interdisciplinary research the above model has two fundamental differences. First, interdisciplinary research is not so much a matter of global cooperation between disciplines, but much more specific: *cooperation between research-programs*. Second, it is a matter of *asymmetric* cooperation: one program poses the problems, the others try to supply solutions, and if successful they have the last word.

If the guide-program itself has already passed its test phase, it is usually directed to science external problem areas of technological or societal nature (compare the Starnbergers), for example environment (9), education, old age (10), cancer, etc.

The main use value aspects of the IR-model are obvious.

(a) Hypothesis: failure to start successful interdisciplinary research may well be due to the collision of cognitive and social factors: against the necessity of asymmetric cooperation there is an inclination to as much symmetry as possible: all participants are expected to deliver the same kind of contribution.

(b) Interdisciplinarity turns out to be less deep and elusive than many philosophers of science have thought.

(c) It is possible that all interdisciplinary research directed to some science external problem area develops such that one discipline provides all guide-programs, whereas the other participating disciplines provide only supply-programs (hierarchical model), but it is also possible that there arises on the level of disciplines a more symmetric situation (interaction model). Hypothesis: on the level of science and research policy, in setting up long-term strategic interdisciplinary research in some science external problem area, it seems very important to start with the interaction model. The reason is that it is easy to imagine that (internal) scientific reasons gradually lead to a hierarchical situation, whereas it will be much more difficult to reach a symmetrical situation starting from a hierarchical one.

For completeness I conclude with noting that the IR-model does not seem appropriate as point of departure for the investigation of a science external problem area when one wants to have short-term practical results. In planning the latter type of research ad hoc considerations seem to be unavoidable.

4. Theory development guided by interesting theories

The time that economists thought that economics could and should in principle be done along Popperian methodological lines has passed, but the question remains how economists do in fact their job.

Bert Hamminga (11) studied the development of the theory of international trade in the period 1930-1970 and came to the following diagnosis. Economists direct their attention to theorems which they find interesting and they try to prove them for an increasing number of conceivable cases, probably with the following motive in the back of their mind: to increase the plausibility that the theorem also holds in the real world. Apart from this motive, however, the real world does not play any role: it is all mathematics. Nevertheless, or precisely because of this, one can find very much systematics in what the economists are doing more in detail.

First of all it is possible to systematize the specific claims of the research program still nicer than Lakatos could have dreamt of: under such conditions it is possible to prove the interesting theorem (IT):

$$V_{lmn} (C_1 \dots C_i; C_{i+1} \dots C_j; C_{j+1} \dots C_k \rightarrow IT)$$

The division of conditions here is as follows. V_{lmn} indicates the *field conditions* which describe the domain of the claim; in the example they are the number of countries, goods and production factors in consideration. $C_1 \dots C_i$ indicate the *basic principles*, i.e. the principles of the general research program with which the problem area is attacked. In the case of international trade this basic program is neo-classical economics, of which the core consists of utility theory. $C_{i+1} \dots C_j$ indicate the *specific principles* for the problem area, e.g. that the production functions are the same in all countries, while the endowments of production factors may differ very much. Finally, $C_{j+1} \dots C_k$ indicate *special conditions* of mathematical nature, e.g. that the production functions are continuous.

Theory development or, more precisely, results which are considered to be important theoretical achievements consist of new specific claims, and their proof, in which only field and/or special conditions have been liberated in one or more of the following ways:

- field extension: increase of the number of countries, goods or factors (the point of departure is always 2 for each!)
- weakening of special conditions
- more plausible conditions for special conditions
- alternate special conditions

Whether economists rightly consider such developments to be real progress because they increase the probability that the theorem holds in the real world, remains a question. However, there can be pointed out (12) a certain formal analogy with progress in the natural sciences, and also the point where the analogy stops.

The picture that Hamminga draws seems representative for neo-classical economics; a recent reconstruction of the theory of the market (12) confirms this diagnosis.

Of course it is not a complete picture of the whole of the economical science, in particular when we take also applied econometric models into consideration, but it does characterize an important part of it, viz. so-called theoretical economics. Moreover it directs attention to the question what the systematics is, if any, in theory development in those areas of economics where the picture drawn seems inadequate.

I confine myself to some short remarks about the use value aspects.

(a) The sketched diagnosis seems to be an important underlying motive for the striking ambivalence of economists about the question whether economics is a social science or not.

(b) The cognitive aims of the social sciences in general and of economics in particular appear to be less evident than philosophers of science use to assume on the analogy of the natural sciences.

(c) The didactic advantage of a structured presentation of (new) specific claims seems obvious. Moreover, it seems useful in general to be straightforward, if possible, about the nature of economics.

(d/e) Heuristic use in research and research policy has not yet been made, as far as I know, but it is easy to imagine.

5. Theory development guided by idealization and concretization

Idealization is frequently applied in empirical scientific practice as an unavoidable step in theory formation and formulation. This is certainly true for the natural sciences; in the social sciences and also in philosophy the necessity of explicit idealization is not yet generally accepted. As a side-remark I like to stress that this holds in particular for current Anglo-American philosophy of science, where most of the popular debates are very far from serious attempts at disciplined and stepwise scientific research.

Surprisingly enough, on closer inspection Marx develops his ideas in *Das Kapital* rather systematically according to the 'method of idealization and concretization'. Leszek Nowak (13) has pointed out this procedure used by Marx in particular by showing how Part I and Part III in their succession can be seen as illustrating what Marx used to call 'rising from the abstract to the concrete'. In itself Nowak's reconstruction work is already an example of an illuminating cognitive study of science. But also the general scheme developed by him turns out to be very useful in the elucidation of textbook theories and explanations.

The general idea is that it is frequently possible to make an ordering in the degree of importance of all factors that influence the value of a certain quantity G , which may even lead to a division in primary and secondary factors. Starting from such an ordering of factors $f_0, f_1, \dots, (f_m)$, in the n -th stage of concretization the factors f_0 up to f_n have been accounted for, while the remaining factors are still neglected:

I&C: if $f_0 \neq 0, f_1 \neq 0, \dots, f_n \neq 0$ and $f_{n+1} = 0, f_{n+2} = 0, \dots$

then $G = G_n(f_0, f_1, \dots, f_n)$

In the 0-th stage there is maximal idealization and when all factors have been concretized, assuming that this is possible, maximal concretization has been achieved. Note that, although it is formally arbitrary when the functional representation of a factor gets the value 0, the neglect of a certain factor is empirically

speaking usually not arbitrary, in which case the functional representation can be chosen in accordance with this.

I&C can be used as an instrument in structuring theories in their research stage as well as in textbooks. Although it seems very plausible to do so, to say the least, it is very surprising that it seldomly is explicitly done. However, in general expositions about what one has been doing or how one should do it, there is frequently reference to I&C, and the transition from the ideal gaslaw to the law of Van der Waals is mentioned as the paradigm case. This transition looks as follows in a stepwise decomposition (where I restrict myself to the crucial formulas and assume the meaning of the symbols to be known):

$$(0) P = RT/V$$

$$(1) P = RT/V - a/V^2$$

$$(2) P = RT/(V-b) - a/V^2 \quad (\text{or: } (P - a/V^2)(V - b) = RT)$$

In the *Poznan Studies for the Philosophy of the Sciences and the Humanities* many examples have been worked out, in particular from biology, economics and sociology. The following considerations about the use value are also based on these examples.

(a) Hypothesis: in the social sciences there is much more social pressure than in the natural sciences to avoid very strong idealizations: fear for the blame to distort reality too much seems to be very influential. More in general it can be said that in the social sciences the discipline to have in a certain domain of inquiry only a few (interacting) research programs is badly developed. In the natural sciences this fruitful discipline has certainly partly been caused by the high costs of experimental research.

(b) The ontological questions raised by I&C seem on closer inspection less interesting than they may appear at first sight.

(c) The above mentioned standard example leads to a very interesting question concerning explanations: is it possible to reconstruct the explanation of a (more) idealized law. Detailed analysis (14) not only shows that the naive kinetic explanation of the law of Van der Waals but also the standard textbook explanation is presented as a concretization of the kinetic explanation of the ideal gaslaw in a fundamentally wrong way. Moreover it is shown in (14) that a conceptually decent concretization is possible if one follows the "I&C-heuristics" systematically.

(d) In setting up a "non-marxian" historical materialist theory Nowak (15) has explicitly used the "I&C-heuristics" as a guide.

(e) What has been said in point (a) gives at the same time some rough indica-

tions for investigation of the program structure of social science research that may well lead to very useful results for research policy.

6. *The structuralist perspective and truth approximation*

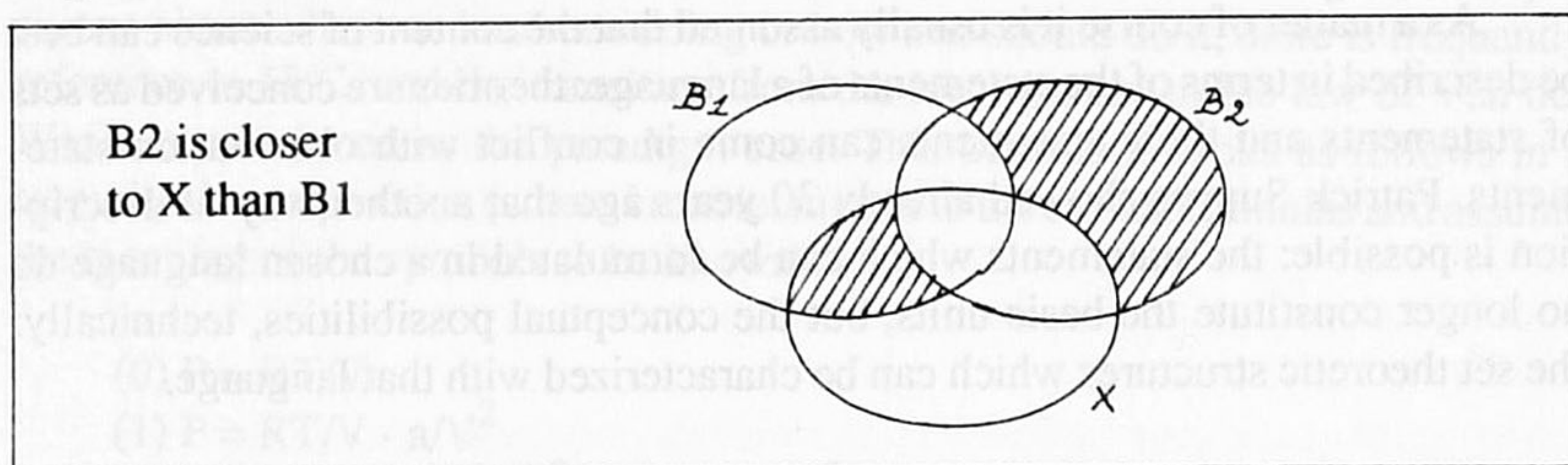
As a matter of course it is usually assumed that the content of science can best be described in terms of the statements of a language: theories are conceived as sets of statements and these statements can come in conflict with observation statements. Patrick Suppes showed already 30 years ago that another way of description is possible: the statements which can be formulated in a chosen language do no longer constitute the basic units, but the conceptual possibilities, technically: the set theoretic structures which can be characterized with that language.

From this perspective it is possible to defend the following meta-theory about the natural sciences at least as an idealized point of departure. Within the set of M of *conceptual possibilities*, designed to characterize a certain domain of phenomena or systems, there exists a unique, constant subset X of all *empirical possibilities*, which constitutes the aim of theory directed scientific research, in other words it is the great unknown looked for. A general hypothesis is a combination of a subset A of M and the claim " $X \subseteq A$ ", while a theory is a combination of a subset B of M and the stronger claim " $X = B$ ". Experimentally realized possibilities R are of course realizations of empirical possibilities: $R \subseteq X$. Hence, $R - A$ contains precisely the realized counter-examples of hypothesis A . If they do not occur there may come a time that hypothesis A gets the status of a law, i.e. the claim " $X \subseteq A$ " is accepted, in which case theory B should explain this law if possible, which is demonstrably the case when $B \subseteq A$.

The success of a theory forms the complement of its problems: the counter-examples and the unexplained laws. The "rule of success" (cf. L. Laudan), stating that scientists prefer the most successful theory, can now be formulated in a very precise way.

This brings us to the notorious question of an explanation of the success of the natural sciences. More precisely: why are current theories as a rule more successful than their predecessors? To this question the answer has become trivial (cf. B. van Fraassen): because scientists apply the rule of success. Hence, the interesting question is another one: why is the rule of success productive, i.e. why do more successful theories usually remain more successful (also when new experiments have been performed or new laws to be explained have been discovered)? The

answer is: because the more successful theory will usually be closer to the truth in the following well described sense: theory B2 is closer to the (unique!) true theory X than theory B1 if the symmetric difference between B2 and X is a proper subset of that between B1 and X. If this is the case then it can be proved that B2 will always be at least as successful as B1 and will even become more successful in the long run.



The indicated structuralist theory of truthlikeness has been developed in a number of publications (16). In the meantime the theory has been concretized in two respects: roughly, not all counterexamples should be treated as equally important (17a) and the theory-relative distinction between theoretical and non-theoretical terms should be included (17b).

The theory itself is a genuine (meta-)theory, with X as its own theoretical notion, and it raises many new questions and suggestions.

(a) Hypothesis: controversies about the nature of the social sciences can be elucidated by starting from the question whether (subject dependent) unique, constant sets of empirical possibilities can be considered as their cognitive aim.

(b) Beside the sketched solution of the verisimilitude problem, the theory turns out to throw also new light on the correspondence theory of truth (18).

(c) As Ronald Giere (19) showed, the claims of specific theories and the nature of counterexamples can be presented in textbooks in a very clear way if one uses the naive structuralist perspective. Unfortunately, Giere has not yet understood what explanations are from this perspective.

(d/e) In research and research policy social sciences are usually treated on a par with the natural sciences. This can only be defended in a rigorous way if the cognitive goals are more or less comparable, and this does not seem to be plausible at all.

7. The structure of theories

As is well known Joseph Sneed has enriched the naive structuralist perspective of Suppes with the theory-relative distinction between theoretical and non-theoretical terms. A theory T has according to Sneed the following ingredients:

M_P :	the set of potential models, i.e. structures in which all relevant terms occur,
M_{PP} :	the set of partial potential models, i.e. the structures with only non-theoretical terms,
M_T :	the set of models of T , $M_T \subseteq M_P$,
pr :	the projection function of M_P onto M_{PP} , deleting the theoretical terms,
I :	the set of representations of the intended applications of T , $I \subseteq M_{PP}$.

The claim of the theory now states that I is a subset of the projection of the models, i.e. of $pr(M_T)$.

The background of the distinction of two levels is the following. For many theories the *prima facie* claim, stating that the intended applications represented as potential models are in fact models, is either circular or leads to an infinite regress, because the application of some terms requires the (partial) validity of that claim.

Note that M and X of Section 6 roughly correspond with M_P and I if we neglect the level distinction; the claim then reduces to the claim of "hypothesis M_T ".

From the sophisticated structuralist perspective several informal questions, concepts and distinctions can be explicated. For example the question whether a theory has empirical content, i.e. whether the sophisticated claim is not generally valid. This leads to the question of the possible use value of conceptual theories, i.e. theories with genuine theoretical terms, but without empirical content. Moreover, concepts like paradigm (Kuhn), research program (Lakatos) and synchronic and diachronic theory nets (Sneed) can be circumscribed in a clear way.

A typical Groningen result (20) is the explication of the intuitive distinction between genuine theories and empirical laws, depending on whether the relevant candidate does or does not have theoretical terms of its own. Using this criterion it turns out that the ideal gaslaw is an empirical law, in contrast to what one might think at first sight on the basis of the fact that the notion of empirical absolute tem-

perature is evidently laden with the ideal gaslaw. Moreover, the distinction throws new light on the discussion about the question whether the Periodic Table is an empirical law or a genuine theory (21). Before the explanation of the periodic law by the atomic theory, it was a genuine theory, with the atomic number as theoretical term. However, the atomic theory gave in principle room for methods of measurement of the atomic number independent of the periodic law (based on the identification with the number of electrons or protons). By consequence, the periodic law became an empirical law, laden with the atomic theory, to which it can also be reduced, in such a way that the above mentioned identification becomes the crucial reduction step (see Section 1).

In Section 4 it was already suggested that the structuralistic reconstruction of the theory of the market (12) is grist to the mill of Hamminga's development model for theoretical economics. In (12) it is also shown that the theory of the market is used in applied economics in a very problematic way, whereas adequate ways of application remain unused.

Some use value aspects were already hinted upon.

(a) Hypothesis: stagnation in an area can be caused by the fact that it is unjustly assumed that a theory has empirical content.

(b) The so-called problem of theoretical terms has found a very practical solution.

(c) The reconstructions in (12) and (21) pretend among others to be useful for improving the structure of the textbook presentations of these theories, comparable to the way in which Giere (19) used the naive reconstruction by Suppes of Newton's particle mechanics.

(d) A new impulse for applied economics was already suggested.

(e) Hypothesis (in the line of Sections 3 and 5): research programs provide adequate (meso)-means for research policy, in particular when they can be interpreted in the structuralist sense, when they ask for idealization and concretization, and when they are presented with interdisciplinary claims.

8. From material to an industrial product

Pieter Weeder and Do Kester (22) have shown convincingly that there is very much system in the construction of an industrial product, in particular in the so-called project phase. The paper is based on the so-called Tenax-case, which seems to be rather representative. On closer inspection this structure turns out to be to some extent analogous to approaching the truth in the sense indicated in Section 6.

Weeder and Kester sketch the construction process as an ordered series of attempts to map the properties of the given material and the requirements of the intended application onto each other. However, I prefer to speak of increasing the overlap between the *factual properties* *F* of the material and the *desired properties* *D* of the intended product. This presupposes that it is always possible to choose such a level of abstraction that all relevant properties belong to one field of properties and that the description is dichotomous: each property is or is not present. But we do not have to assume that the properties have to be empirically independent.

In this way *F* and *D* form in each stage subsets of the field and in general they will differ: the symmetric difference is non-empty and the question is whether and how this difference can be reduced to an empty set.

Weeder and Kester show on the basis of the Tenax-case, that in such a situation one first tries to apply one of the standard methods in order to change the properties of the material. One possibility is of course that this can be done in a successful way, in which case the new set of factual properties *F* has a greater overlap with *D* and *F*, whereas the remaining symmetric difference characterizes the new problem situation. Especially due to the empirical dependence of properties it is however frequently the case that successful transformation of one property causes new problems with other properties that did not exist before. For the new problem definition there has now to be chosen out of two options: accepting the new problem or looking for another way out of the old problem situation.

When it turns out not to be possible to proceed with the standard methods, Weeder and Kester speak of an anomalous problem asking for a strategic decision. In total they distinguish eight possibilities, which can briefly be summarized as follows: (1): stop the whole enterprise, (2, 3, 4): choose another material and/or another application, such that the factual and/or the desired properties change, (5, 6, 7): change the factual and/or desired properties, but stick to material and application as much as possible, (8): postpone a strategic decision.

The analogy with truth approximation is of course the following. In the same way as one tries in the industrial laboratory first to let *F* move in the direction of *D*, the theory directed empirical scientist tries to move the conceptual possibilities *B*, selected by his theory, in the direction of *X*. The instructiveness of the formal analogy lies of course at least as much in the differences that become clearly visible:

- X is an unknown, while D is known,
- by consequence, whether a step has been put in the direction of X, or of D, respectively, cannot, respectively, can be judged in a direct way,
- X cannot be changed, D can be changed to some extent without changing the intended product and it can be changed rigorously if the intended product may change,
- formally speaking, it is relatively easy to change B, but it is in general difficult to change F.

From the foregoing three things have become clear. The most important thing is that Weeder and Kester showed that there is much system in the process of industrial product and hence knowledge formation. Consequently, cognitive analysis of this process is very well possible. It should be mentioned that they pay also very much attention to the variation and selection aspects of the process. Moreover, in this short presentation it became already clear that their analysis cannot only be formalized to some extent, but also that this leads to a certain analogy with the development of scientific knowledge.

Whether the last two points are really useful, where one might think in particular of the use value aspects (a), (c) and (d), I leave here as an open question.

9. Similarity and the choice of an inductive statistical method

Since Plato and Aristotle the concept of similarity is used in science and philosophy. In contrast, one finds in the respective sciences many ad hoc definitions. This is unfortunate for it will be clear that a general explication could lead to general qualitative and quantitative measures of correspondence and difference between comparable matters.

Roberto Festa has shown in a number of papers, culminating in (23a), how the concept of similarity can be explicated.

Subsequently he uses this explication, in critical discussion with current practices, in the analysis of philosophical and statistical problems.

More precisely, in (23a) he gives a critical-historical survey of the use of informal similarity notions in philosophical research and an inventory of different quantitative similarity measures which are used in statistics, in particular in biology and mathematical psychology.

The core (also contained in (23b)) consists of a formal analysis of the similarity notion and, based on this, a network of similarity measures with various properties. Subsequently, he investigates the usefulness for a number of subjects:

- (truth-)likeness of qualitative and quantitative theories,
- interval estimation based on the distance between an interval and a point hypothesis,
- analysis of the notion 'degree of (dis-)order', with several methodological and statistical implications,
- inductive probabilities and the choice of inductive methods.

I will confine myself to an impression of the last subject. The problem is the choice of a specific method out of a general class of inductive methods, all of which are directed to approach the objective chances of each of a finite number of categories in a infinite universe. It is useful to formulate first the logical-metaphysical problem of optimality: suppose we would know the objective probabilities, is there in that case an optimal inductive method? In his *Continuum of Inductive Methods* (1952) Carnap showed directly that, if the question is restricted to his so-called continuum, the answer is, surprisingly enough, positive. Festa (24) does not only show that this positive answer can be generalized to the class of so-called 'generalized Carnapian (GC-)systems' (25), albeit with a different specific solution, but also that this optimal behaviour can be reinterpreted as the maximization of the similarity between the inductive and the objective chances (defined in terms of their distance). In this analysis the so-called diversity index of Gini plays an illuminating role.

In the line of this Festa starts in his (24b) to tackle the epistemological-methodological optimality problem: is there a plausible choice of method possible if we do not know the objective probabilities? In what follows all estimates are informal, external estimates, not to be confused with (mathematical) expectations/expected values based on some probability function. Point of departure form initial (relatively a priori) estimates of the objective probabilities and an estimate of their diversity. Under very plausible conditions this estimated diversity will be smaller than the diversity of the estimated probabilities. For instance, the background knowledge may well justify the assumption that the diversity will not be maximal, without giving any indication in what direction. The latter leads to equal initial estimates, and this leads on its turn to maximal diversity of the initial estimates. Moreover, it will be possible to calculate an estimate of the distance between the objective probabilities and their estimates. Now it is plausible to choose that inductive system, starting with the mentioned initial estimates, which would be (logically)

optimal if the objective probabilities would be such that the corresponding objective diversity and distance would be equal to the estimated diversity and distance. This choice turns out, moreover, to be the only consistent choice in the sense that there is no other GC-system of which the (mathematically) expected value of the diversity is equal to the estimated diversity. In other words, in the described circumstances two plausible criteria which seem conceptually quite different lead to the same choice of the crucial parameter λ :

for objective probabilities $p_i (\geq 0, \sum p_i = 1)$
 and initial estimates $e_i (\geq 0, \sum e_i = 1)$
 $G =_{df} 1 - \sum p_i^2$ is the objective Gini-diversity,
 $D =_{df} \sum (e_i - p_i)^2$ is the objective distance to the initial estimates,
 and Estimate (G)/Estimate (D) is the plausible value of the λ parameter.

This proposal is more useful (in sense (d)) in statistical practice than one might think at first sight, because in many cases it is possible to make reasonable estimates of diversity on the basis of background information, e.g. in ecology. Even if, besides, such an estimate, one cannot think of something better than equal estimates for the separate probabilities (leading to maximal diversity of these estimates!), then it is already possible to motivate a very specific choice of inductive statistical method. In other words, the conceptual investigation of similarity in relation to inductive methods, has led to a plausible choice which is in any case better motivated than the usual, ad hoc choices, to say the least.

Concluding remark

As already announced in the introduction the presented survey puts strong emphasis on Groningen (affiliated) research. Elsewhere in and outside the Netherlands there are certainly tendencies in the direction of cognitive studies of science. Instead of trying to give a sketch of these signs, I like to conclude with pointing at a whole area of cognitive study of science *avant la lettre*: systems theory, of course restricted to its philosophically modest forms. As is well-known just the concept of a system plays already a very useful, structuring role.

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